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## CONTENTS



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## THE PSYCHOLOGICAL REVIEW

**Johns Hopkins University, Baltimore, Maryland**

VOL. XVIII. No. 3. May, 1911.

THE PSYCHOLOGICAL REVIEW

# A WORKING HYPOTHESIS FOR INNER PSYCHOPHYSICS[1]

BY RAYMOND DODGE

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## I. PSYCHOPHYSICAL PARALLELISM

Notwithstanding my conviction that a modified psychophysical parallelism is the least objectionable of the current concepts by which we can express the correlation of noetically mutually irreducible systems of experience, I believe that it has been and is an unfortunate working hypothesis for psychological investigation.

In the history of inner psychophysics, psychophysical parallelism has been notably sterile. In a period of relatively rapid development of psychological and physiological experience and theory, one seeks in vain for a single advance in inner psychophysics that can be accredited legitimately to the principle of psychophysical parallelism. On the contrary, in four respects at least it is a distinct handicap.

1. Aside from its metaphysics, with which psychology is only indirectly concerned, the principle of psychophysical parallelism is inhibiting rather than stimulating.

If at the outset of psychophysical investigation we postulate a thoroughgoing parallelism between the phenomena of introspection and nervous physiology, we thereby abstract at the beginning from important questions of fact. Psychology as empirical science may not shift its scientific responsibilities by a noetical construction. The correlation of nervous processes and consciousness is phenomenal not

[1] Read in part at the meeting of the New York Branch of the American Psychological Association, February 4, 1911.

noumenal. It is a problem of experience before it can properly become a problem of theoretical implication.

Now as a matter of actual experience it is conspicuous that the two series are not parallel throughout.

The phenomena of consciousness cease to exist in the only form in which they are given to us directly, while some of the processes of nervous metabolism may be demonstrated to continue. Furthermore, there seems to be little doubt that consciousness as it is given to us directly is more intimately and directly connected with the activity of the superior cerebro-spinal ganglion than with any of the others. But if consciousness, in the only form in which we can know it directly, is correlated with specific nervous processes in specific parts of the nervous system, it is part of the business of inner psychophysics to discover the peculiar characteristics of the neural processes which appear to be necessary conditions of that consciousness which we know.

This is a plain straight-forward scientific problem. Towards its solution much remains to be done. But it is obvious that the advance will not be stimulated by the belief in a thoroughgoing parallelism.

Perhaps each segment of the spinal cord, or even each neuron or physical atom, may possess a consciousness of its own. The supposition is entirely thinkable. It is, moreover, apparently incapable of disproof. But a suppositious cord consciousness is at present utterly inaccessible to us. It is not our consciousness. It is not even comprehensible in terms of our consciousness. The hypothesis of a special kind of inaccessible consciousness to correlate with every kind of neural process is utterly indefensible as empirical science. While it may be justifiable on philosophic grounds, as a psychological hypothesis it merely confuses the issue. It would be legitimate only if we could show that the various neural processes actually possess those characteristics which in the cerebrum are correlated with consciousness as we know it.

Psychic units and psychic elements never have been, and never can be correlated with physiological units and ele-

mentary physiological processes. A psychic element always appears as the resultant of a physiological manifold. A color or tone which for consciousness is not further analyzable is physiologically still highly complex. Beginning in the sense organ and ending in the cortex there are at least three links in the chain of neural happenings. In the case of color the number of links is probably higher. But in none of the links is the process a simple one. It is impossible to start the process in a single receptive element without interaction with some of the other receptive elements of the receptive organ. In the last link it spreads to a more or less general cortical disturbance with complex interplay of excitations and inhibitions.

It may be that some simple conscious process parallels the migration of each separate ion in nervous tissue. But such conscious processes are utterly inaccessible to us. We can form no conjecture from our inner experience as to what they might be like. They belong to that large but discredited class of hypotheses, that spring, not from the facts, but from the imagination. Their main function is to support an antecedent hypothesis. They are utterly unprovable and irrefutable. They have no proper place in a science of phenomena. On the contrary it is clearly a part of the business of psychology as science to ask, what is the peculiar complication of the physiological manifold whose correlate is actual sensation.

2. Both in the *Elemente* and in the *In Sachen der Psychophysik*, Fechner promulgated the now widely accepted doctrine that the 'threshold' of outer psychophysics has a counterpart in inner psychophysics. It seems to be generally agreed that even the peculiar nervous correlate of normal consciousness may be stimulated sub-liminally. Fechner held that the sub-threshold waves have a definite configuration which represents their mutual interdependence and determines their advent into consciousness. The same view finds various expression in current psychological theory.

Through its various practical and theoretical bearings on normal and abnormal mental life, the problem of the sub-

conscious excitations has become vital. Psychology cannot ignore it. But by the same token it must deal with the facts, not with mere speculations about them. I for one must refuse to believe on any merely analogical grounds that the accessible phenomena of consciousness are related to the unconscious as quantitative variations of the same functional facts. There seems to me to be a clear organic difference. It is utterly incomprehensible to me that consciousness is any mere 'emphasis' on the unconscious. Certainly there is no conclusive evidence for the supposition in the argument by analogy from external stimulation. No one really holds, as far as I am aware, that consciousness as such is directly correlated with a quantitative change in the stimulation of a sense organ. The nervous effect of supra-threshold stimulation is probably qualitatively different from that of sub-threshold stimulation. There is abundant analogical evidence in the response of living tissue to various chemical, electrical, and mechanical stimuli, that quantitative changes in the stimuli produce qualitative changes in the reactions, even to a 'reversal' of their 'sense.' There is direct physiological evidence to the point in the regular close relationship of relative fatigue, inhibition, and the refractory period, with normal reaction, 'Treppe' and 'Scheinbare Bahnung.' There is direct psychophysical evidence in the qualititave changes of sensation and feeling through quantitative changes in stimulation.

But even if the incomprehensible could be granted as demonstrated fact, psychophysical parallelism would still need limitation and modification before it would fit the phenomena of inner psychophysics. Our consciousness is obviously not correlated with the sub-threshold intensity of action of the nervous system, but only with a nervous activity of definite, *i. e.*, of supra-threshold intensity. We must face the phenomenal problems: What really happens when the threshold is reached? Why do supra-threshold intensities alone have conscious correlates in the only form in which we can know them directly? These questions, however, are matters for scientific analysis, not for speculation. And the analysis must be free from speculative prejudice.

3. Further evidence for the inadequacy of parallelism as a working hypothesis for inner psychophysics is the fact that it breaks down when we really use it, and leads to absurdity. We are utterly unable to reason successfully either from known nervous facts to consciousness, or from consciousness to its nervous correlates. Phenomenal parallelism as a working hypothesis for inner psychophysics assumes too much. It is equally embarrassed by the question where in the scale of organic existence a consciousness comparable to our own begins, as well as by the question concerning the specific conditions of the only consciousness which we can know directly.

Wherever psychology has ventured to express itself in supposedly parallel physiological concepts it has made a lamentable failure. At the present time at least we cannot use the terms quantity, force, work, fatigue, inhibition, excitation or facilitation with any proper physiological connotation in psychology. For example, we know nothing at all what a fatigue of consciousness might mean. While a consciousness of fatigue is a very different matter from physiological fatigue. Conversely animal physiology is just now seriously trying to rid itself of anthropomorphism in its concepts and in its terminology.

In short the hypothesis of psychophysical parallelism is not only sterile as a working hypothesis but it must be tinkered before it will fit the facts. It cannot be trusted anywhere, and is consequently altogether unfitted for the functions of a working hypothesis.

4. Finally, parallelism in inner psychophysics is really at heart a confession of scientific impotence. DuBois Reymond's 'Ignorabimus' may be the final word. But it was never a satisfactory starting point for developing science. It may be that 'astronomical knowledge' of neural processes will forever fail to discover consciousness. Meantime, however, it is pertinent and legitimate to ask what characteristics of the nervous processes are essential to our normal consciousness as it is immediately given to us in introspection.

The task of every science is to describe the sum total

of the conditions of the various phenomena that it investigates. Psychology has no especial privilege if it would be a science, even though the task reaches beyond our present strength.

Some of the conditions of the various phenomena of consciousness seem to be given in introspection. In no case are they completely given. Consciousness is always something more than the sum of the factors that we can introspectively analyze out of it. Wundt's principle of creative synthesis[1] must be admitted even in a descriptive psychology.[2] It is true of the simplest combinations of sensations. It is still more conspicuously true of meaning and personality.

The conditions of consciousness itself can never be accessible to direct introspection, since as conditions of consciousness they must also be conditions of introspection. Obviously direct analysis can reach only so far as introspection reaches. If in spite of the paralyzing dogma that consciousness is unanalyzable and undefinable, we persist in asking the scientific question concerning the conditions of consciousness that makes introspection possible, we must use some other technique.

Similarly, analysis of the neural process will doubtless never reach beyond itself. Certainly physiological analysis by itself will never reach the psychical, since our human consciousness is correlated not with elemental metabolic processes but with a specific organic complication of metabolic processes in a specific organ.

From both the physiological and the psychological standpoints the problem of consciousness appears as a synthetic one. We must ultimately discover what the organic complications involved in consciousness actually are. It will do us little good to catalogue the elements of which it is composed.

It is the business of a profitable working hypothesis to point out a direction of investigation that looks promising. Under the present circumstances it seems to me that the first step may well be to make some reasonable estimate of the

[1] *Phil. Stud.*, X., 1–124.

[2] Ebbinghaus, *Zeit. f. P. u. P. der S.*, IX., 161–205.

kind of organic integration that would approximate consciousness as we know it.

## II. The Principle of Apperceptive Integration

While it is probable that the integrative principle of consciousness can never be separated out of consciousness by introspective analysis, since in all consciousness it is presupposed not as a factor but as consciousness itself; nevertheless, if we are to discover any clue to the integrative principle it must be from consciousness. We may reasonably expect that it will be reflected in the fundamental organizations within consciousness that are accessible to us.

If we should ask what has generally appealed to scientific psychologists as the most essential characteristics of consciousness, memory and association would probably be the favorites by a large margin. Without either, consciousness as we know it could not exist. Modifications of either by accident or disease effect grave changes in the soul life, not in its stuff merely but in its organic character.

In what seems to most psychologists an extreme and untenable position, Loeb holds that associative memory is consciousness. The psychological offence in Loeb's formulation consists in an apparent petitio principii. Psychology knows no association except the association that may occur between facts that are already conscious. Similarly in memory, what is remembered must have previously been a conscious fact. But it gives no real clue to the nature of consciousness to say that consciousness is the revival and association of conscious facts. We are bound to ask the further question: What constituted consciousness in the original of the associated and remembered fact?

I suppose that Loeb's answer to the difficulty would be to insist that both memory and association are wide embracing biological concepts. That memory is a function of all organic matter and that association is equally a characteristic of all living tissue. But in this sense, *i. e.*, the only sense in which Loeb's principle ceases to be a petitio principii, it ceases to be strictly true. If association and memory are general

biological principles, we must demand the peculiar characteristics of the associative memory that constitutes our consciousness. These characteristics it seems to me are given in the type of associative reproduction which for want of a better name we call apperception.

In using the word apperception we must make it clear that we are not using it in the restricted sense of Wundtian theory but rather in the more general sense indicated in Erdmann's apparently little known but eminently sound description of the facts.[1] By apperception then we mean that complex reproductive process that occurs in every perception of developed consciousness, in which the present stimulus immediately arouses and fuses with the residua of similar past stimulation, and mediately arouses and is interwoven with the residua of past experiences which are associated with it.

But in order to use even Erdmann's formula as the schema for all conscious integration we must abstract from the specific function in whose service the apperceptive process is here exploited, and from those characteristics of the apperceptive process which relate only to direct sense perception. Our generalized principle might be restated as follows: Apperceptive integration is a general name for a complex reproductive process which occurs in every moment of developed consciousness, in which the present nervous excitation in so far as it comes to consciousness at all, arouses immediately the residua of previous similar excitations and fuses with them; while it also arouses mediately the residua of previous excitations originally connected with it.

The question of the validity of our working hypothesis is a triple one: Could such an integrative principle give us the organization of consciousness as we know it? Does it correspond with the psychological facts at our command? Does it correspond with our physiological data?

## III. Hypothetical Questions in Synthetic Psychology

If we postulate a living tissue of such character: (1) that $n$ changes in its environment ($nS$) tend to excite $n$

[1] *Vierteljahrschrift für wiss. Phil.*, Bd. X., pp. 307 and 391.

characteristically different reactive modifications in the life history of its elements and their organic interrelations, ($nR$); (2) that every new $S$ tends to reproduce every previous $R$ in definite sequences, though in various degrees of completeness; (3) so that every new $R$ becomes a part of a relatively slowly changing system of reactive modifications, with which it thus becomes organically integrated and to which it adds its peculiarity: should we not with such a reproductive organization of its reactive modifications grant to our tissue a kind of consciousness, in which the qualities of the original $nR$ are logical accidents, depending on the number of discrete reactive modifications of which the elements of our hypothetical tissue are capable, within the limits of their organic interrelations?

If we further postulate a circular excito-reactive process by which the directly excited factors of the system tend to reinforce the $S$ which aroused them, and to emphasize the corresponding $R$, together with other $R$s immediately connected with it; while it tends to inhibit less directly connected processes, within the limits of relative fatigue; would we not therewith grant our tissue a kind of selective attention?

If we further postulate a relatively slowly changing group of emphasized $R$s which are regularly reproduced with every new $R$ must we not therewith grant our tissue a kind of personality?

## IV. The Psychological Problem

As is indicated by their form, our questions make no pretence of giving an elaborated psychological theory. They aim to focalize the problem of the organic structure of consciousness, and to suggest a synthetic solution of the problem.

The fundamental principle which underlies the questions, *i. e.*, that consciousness may be known as a kind of organization, is by no means new. Psychology at the very beginning of its modern period undertook responsibility for analyzing the complex forms of organization which are directly observable within consciousness. It has made substantial additions to positive knowledge, not only in the fields of perception

memory and association, but also in the more complex fields, of language and secondary identification, meaning and co-ordinated response, and the thought processes themselves. The limits of progress seem to be determined only by the precision of our problems and the development of adequate technique.

Our thesis however goes further. Not only are there discoverable forms of organization within consciousness which characterize the systematic groupings of experience into units, temporal and spatial, practical and scientific, logical ethical, and religious, etc., but we hold that consciousness itself is not essentially different from the observable phenomena of consciousness. It is conspicuous that we can never catch consciousness except in organized form. Single conscious elements never have been discovered, and never will be. They are scientific abstractions. Moreover there is abundant and excellent psychological precedent from Kant to the present, for believing that consciousness itself may be cognized as a kind of organization.

The lamented James put the matter in his striking way: "Consciousness connotes a kind of external relation, and does not denote a special stuff or way of thinking." "The peculiarity of our experiences, that they not only are but are known, which their conscious quality is invoked to explain, is better explained by their relation to one another."

The thesis is not lacking direct scientific evidence. The splendid experimental analyses of hysteria and the subconscious, of divided consciousness and multiple personality clearly indicate that any actual unity of consciousness is neither intrinsic nor necessary, but functional. Consciousness never appears as a homogeneous quantum of some specific stuff. It always involves the unification of discrete, more or less highly differentiated factors. Divided consciousness and dual personality have the common characteristic of a disturbance of the unifying process.

Similar phenomena are familiar enough in normal mental life. Adequate stimuli of many kinds are constantly affecting the sensorium. The fact that the vast majority fail to come

to consciousness is not due to any inherent characteristics of the stimuli themselves. The same stimuli under slightly different subjective conditions may give rise to conspicuous features of our experience. In the former case available material failed to be taken up into the momentary organization of cleared consciousness. Sometimes subsequent moments appropriate it. It may affect subsequent moments without ever clearing independently. There is evidence that it may leave revivable residua. Apparently much is lost forever, and never becomes a part of our organized experience. These differences of relationship between consciousness and its material are especially conspicuous in connection with the organic sensations.

Experimental evidence is conclusive that the arousal of nervous residua is no guarantee of corresponding consciousness. Specific residua may be actively stimulated and function in the process of association without clearing up in consciousness. If the actively stimulated residua at one time fail to be included in consciousness and at another time succeed, the difference must lie in something outside the residua themselves. We can figure it in terms of our theory as a variation in the completeness of the interneural processes. The stuff of consciousness was present in both cases. What was lacking in the former instance seems to be the completeness of the organization. The unconsciously aroused residua fail of being taken up into the particular moment of organization and unification in which our present consciousness consists.

Our personal consciousness is clearly a functional unity. If possible material fails to be included, in normal life it will be inhibited; in abnormal conditions it may organize in an independent system.

Unfortunately, in its attempts to differentiate the specific character of conscious organization, psychology has been far from unanimous. A considerable variety of stimulating hypotheses is found in contemporary discussions. This is no place for a criticism of these various theories, even if I had the capacity or the interest to do it impartially. Our

present business is exposition. Critical discussion can follow, if any excuse for it arises.

An important characteristic of the principle of apperceptive integration is that it makes no claim to be a principle of explanation. The particular physiological conditions of organization are left indeterminate. But if consciousness itself, as well as its various aspects can be regarded as a kind of organization, the special problems of inner psychophysics as well as its main problem are rendered more concrete and definite.

The second claim to attention is that our principle is drawn directly from fundamental mental processes which are directly or indirectly accessible to introspection.

Perception itself, and consequently introspection, shows the process in completest form in the processes of apperception and assimilation. Various aspects of the process are conspicuous in the familiar abstractions of conscious memory and association, in habit, in the conscious controls, and in conduct.

In memory and association the application of our principle is obvious.

Habit and conduct have always offered peculiar difficulties to the application of psychophysical formulæ. The degradation of consciousness in the facilitation of habit was always an offence to the motor theories of consciousness. If we once admit, however, that consciousness is a particular kind of organization of organic reactive modifications, then it is evident that a simplification of the organization, whether by the development of direct arcs within the cerebrum, or in the lower ganglia, will lose the peculiar characteristics of the conscious organization. Secondary automatic acts will participate in consciousness in the same way that the simplest reflexes, like the knee jerk and the protective wink participate. Situation and response will be perceived as phases of a single process.

In so-called voluntary activity, on the other hand, with its conscious suppressions and reinforcements, the resulting activity may, and in its highest form will, represent the

resultant of the completest practicable interplay of the individual's past, present, and future experiences. Each relatively steady control adds to the complexity of the regularly reproduced reactive modifications which make up the personality.

Conversely in organisms whose nervous systems contain only relatively simple reflex mechanisms, there is neither ground nor excuse for assuming the complex reproductive processes of consciousness. The application of our principle to the problems of animal consciousness is at least a clear cut issue.

Loeb and others make the criterion of consciousness the educability of an organism. Against this all the force of Becker's arguments against Driesch apply without the possibility of Driesch's answer. Moreover, it is confronted by empirical contradiction. Many of our human reflexes show development and change by repetition without conscious participation.

Miss Washburn suggests that the simplest consciousness consists in a series of flashes. If amœbas have minds that act in a series of flashes, where these minds may be and what they are when they are not flashing are as unexplained and inexplicable as the nature of the flash itself. On the basis of our organic principle we are bound to ask whether the amœba is of such organic complexity that a reactive modification can reproduce past reactive modifications in any definite series however primitive. I believe that the available evidence is against it.

In a single cell all internal reactive modification is at the same time external. But the actual reactions of amœba, though they show adaptation give no evidence of habitual series of differentiated reactions resulting from each new stimulation. Such systems would be a menace to its welfare, or to the welfare of any creature in which external reaction and internal reaction were identical. Only in a differentiated tissue, whose reactive modifications do not find immediate expression in action, would the reproduction of previous modifications in habitual systems be tolerable. Such a dif-

ferentiated tissue seems to be observable only in the nervous ganglia.

A cell consciousness comparable to our consciousness seems improbable. Aside from the difficulty that we can discover no suitable mechanism, the very simplicity of the life history of the single cell leaves no room for complex differentiated experiences. Even if by some hocus pocus a single cell could be artificially endowed with differentiated experiences revivable in definite chains, it would be an utterly useless burden. In the monotonous reactions of its simple life it must soon decay from disuse.

## V. The Physiological Problem

From the standpoint of biology and physiology, I believe that the conception of consciousness under the schema of a specific kind of organization is just as conspicuously an outgrowth of the data. With the epoch-making developments in biological science which began in the middle of the last century, the motives for such a conception were very strong. The early biological theories, however, suffered from the general preëxperimental confusion between psychological fact and psychological hypothesis. Moreover, they often confused the issue with mechanical or materialistic interpretations.

With the discovery of the electrical currents of action in nerve and muscle, theories of nervous organization followed the analogy of the telegraph. The cerebrum corresponded to the central exchange or switch-board. Energy was supposed to enter the system in sense stimulation. Variously combined and modified it found exit as a motor impulse. Consciousness was a product of the switching process.

Traces of this conception are still found in popular thought and even in psychological text-books. But the present scientific view of the action of the nervous system is fundamentally different. Each of the millions of neurons whose various clusters make the anatomical units of the nervous system, seems to be a rather independent little body, and leads a relatively independent life. It has its own supply of energy; regulates to a considerable degree its own nutrition and

growth; reacts in its own peculiar way to the chemical and nervous stimuli to which it happens to be sensitive; and finally, it may die without necessarily involving the destruction of its neighbors. In general each neuron seems to be particularly sensitive to some particular kind of stimulus. But its action in every case seems to be relatively independent of the kind of stimulus that excites it. That appears to be determined primarily by its own specific form of energizing. Each neuron may stimulate an indefinite number of other neurons; and each may in turn be variously stimulated. The end of the chain may be gland, muscle, or nervous tissue. Whatever the final outcome, each step in the process, including the last, is determined by the character of the participant links. The result, as measured at different points of the process, may bear widely different ratios to the energy of the original stimulation. It may become negative in direct inhibition. It may reach enormous proportions by association.

Take, for example, the familiar phenomenon of the knee jerk. Considerable pressure against the tendon produces no reaction. The same energy in the form of a quick tap may develop a vigorous reaction. It is possible to adjust the stimulus so that it develops more than twenty-five times its energy in the reaction. But if the same blows are given in a series of about ten per second, the reactions entirely disappear in a relative refractory phase.[1]

Similarly, ether vibrations of barely threshold intensity, corresponding to the light reflected from a thin column of smoke in a darkened theater, may produce extensive neural excitations. It may lead to the maximum activity of the largest muscles in the body. Nervous activity is no longer regarded as the central reflection and final motor emergence of the afferent energy derived from sense stimulation. Each link in the chain is a magazine of potential energy, which reacts to adequate stimulation in its own way. Every sensory-motor reaction may thus be traced through the nervous system as a chain of stimulations and adjustments.

Somewhere in that process our consciousness arises.

[1] Dodge, *Zeitschrift für allgemeine Physiologie*, Bd. XII., pp. 53–55.

There is evidence that its locus is not in the peripheral sense organ. Though there have been recent attempts to connect it with the basal ganglia, the weight of evidence forces us to regard it as correlated with cerebral activities.

The Cartesian theory of a single point of contact between soul and body is clearly inadequate. It was long ago abandoned as a tenable hypothesis in physiology. But its effects are still felt. True there is no single place discoverable where consciousness as such is located. No circumscribed or focal lesion necessarily destroys consciousness as a whole. But it is not uncommonly held by psychologists and physiologists alike that certain kinds of consciousness are located in discrete cortical areas. Certainly there is no warrant for such an assumption in our present knowledge of the localization of function. It is indeed true that each of the several sensory projection fields has peculiar functions with relation to the rest which no other field can vicariously affect. But we cannot say that a certain kind of consciousness is here or there. If a given field is destroyed consciousness will lose some of its possible variations, but consciousness will remain. It will be a distorted, crippled consciousness, with more or less conspicuous differences from the normal. The brightness and the song will be gone. But a very human consciousness without either vision or audition is a conspicuous reality. The assumption, on the other hand, that the visual projection area contains the correlate of a visual consciousness, which might persist if these areas could be isolated, is without a shred of evidence. The anatomical provision for the intricate organization of each part with all the rest is certainly not meaningless. Its obvious lesson is confirmed by some of the most assured products of psychological analysis, which indicate the complexity of the physiological conditions of even the simplest concrete facts of consciousness.

If some of the present tendencies in physiology appear to emphasize the processes within the cell units, the physiology of the cerebrum never more clearly faced the problem of the importance of the systematic interrelations of the neurons. This is especially conspicuous in the substitution of the con-

cept of a point of maximum vulnerability for the old concept of localization, in the speech disorders. The assumption that consciousness is somehow connected with the individual cells is certainly not based on any clear notion of how the intra-neuron processes are related to consciousness. I conjecture it is rather because it is recognized that the cerebrum is an integrating organ and it is uncritically assumed that the elements must have the same characteristics as the integrated whole.

## VI. The Stuff of Consciousness

The question as to the nature of the unorganized stuff of consciousness is not really a part of our present discussion. But it is bound to be asked and ultimately it is vital. Is there a peculiar soul stuff, or not? For myself I must confess that the problem is tremendously interesting, but it seems to me that this is an added motive to guard against methodological error.

Whatever the answer, as far as science is concerned, it must be in the form of a hypothesis. There will be a characteristic difference between this hypothesis and the hypothesis of our questions. The latter appeared to be not a principle of explanation, but a working hypothesis. The motive for the former is not practical as far as I can see but purely speculative. The question must be asked. It will be answered. The speculative demands of human reason must be satisfied or the completest organization of consciousness will be lacking. But the question and its answer are not directly pertinent to the main question at issue.

On the other hand, our fundamental hypothesis does effect a certain control of possible answers. It is obvious that on this basis, the stuff of consciousness cannot be sensations, feelings, memory images or any of the other direct products of introspective analysis. As far as these facts are not pure abstractions, they obviously serve as building material for the complex organizations within consciousness. They cannot be the unintegrated stuff of consciousness because they are already conscious.

It is widely held that certain moral needs demand that

the stuff of consciousness be a peculiar sort of soul stuff. The demand arises in the effort to escape the determinism of a materialistic universe. These needs certainly deserve a hearing. But it never seemed to me that the hypothesis of a special soul stuff helped matters much when we are forever forced to deal with that stuff and its combinations under the category of causality. Neither does it seem to me that the stuff of our material world-construct can be held responsible for its organization. It cannot supply the principles of its organization without being more than stuff.

One is justly suspicious of cycle and epicycle of hypotheses. There are good methodological grounds for making explanatory hypotheses as simple as possible. In the present case these grounds constitute a motive for the hypothesis that the stuff of our spatial construct, on the one hand, and of our consciousness, on the other, have no necessary and intrinsic differences. The stuff of conscious integration must be capable of that kind of organization. But I can see no intrinsic reason why the same stuff should not be capable of organization in the other series too. I can discover no other necessary peculiarity for the stuff of consciousness than the fact that it is so organized. There is no negation given or implied. As far as I can see the stuff of consciousness might enter into all sorts of different systems, as any given atom in the body is also in systematic relations with the inanimate.

In one very fundamental sense metaphysical parallelism was not affected by our critique of parallelism as a working hypothesis for inner psychophysics. It seems to me that we might well hold that the stuff of the universe shows parallel attributes. That it is at once a soul stuff and a matter stuff. But I can understand this in no other way than one might say that an atom of carbon is either organic or inorganic. As atom it is neither. In the course of history it may be both. But this sort of parallelism makes no pretense to knowledge of the one form of organization from knowledge of the other form. On this basis it would be absurd to hold that the organization of the primitive stuff in consciousness

was paralleled by the organization of some more of the primitive stuff in brain tissue. The concept would be ludicrous and I believe it is ludicrous.

To sum up: My main contention is that the stuff of consciousness is a logical accident. Whatever it were, the right kind of integration would constitute a kind of consciousness. I can see no reason why any stuff in the universe may not enter into a similar kind of organization, if the proper conditions are given.

# THE RELATION OF THE TIME OF A JUDGMENT TO ITS ACCURACY

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Present-day interest in sensory judgments of small differences apparently lies not so much in the determination of the differential threshold and the validity of Weber's law as in the analysis of the factors which condition the judgments themselves. Among the various factors involved in the judgment of liminal differences the time required or allowed for arriving at the judgment is one that has been given little consideration. No doubt the reason for this is the one recently expressed by Titchener[1] that the times of judgment of comparison, etc., "become extremely variable, and have little significance." Judgment times are obviously very variable, but it nevertheless is not improbable that their measurement may serve as a valuable means of analysis and not "merely as a convenient setting for exercises in introspection," just as the measurement of the *Trefferzeit* in experiments on memory has been of value in the study of reproduction and the force of associations. Martin and Müller[2] in their experiments with lifted weights, recorded the times of judgment without the knowledge of their subjects, by counting the metronome beats intervening between the raising of the second weight and the pronouncing of the judgment, the metronome beating at intervals of .7 sec. In summarizing the results of the time measurements, they conclude: "(1) That the time of a judgment is dependent on the psychic factors involved, *i. e.*, whether the judgment follows immediately from the absolute impression of one of two stimuli, from a comparison of the stimuli, from secondary comparisons, or from the coöperation of several such factors;

[1] Titchener, E. B., 'Text-book of Psychology,' New York, 1910, p. 447.

[2] Martin, L. J., and Müller, G. E., 'Zur Analyse der Unterschiedsempfindlichkeit,' Leipzig, 1899, pp. 196–206.

(2) with constancy in the factors producing the judgment and under constant conditions, the time of a judgment is shorter the clearer the psychic factors are; if the judgment depends upon the non-appearance of such factors as 'doubtful' and 'undecided' then the time of judgment is longer than when the judgment is determined by positive factors; (3) the time of a judgment depends on whether the psychic factors concerned are associated in an unambiguous way with a particular category of judgment; if the judgment falls in an intermediate region, *e. g.*, between 'smaller' and 'clearly smaller,' then the time is longer even than in cases when the perception of difference is less clear, but happens to coincide with a definite category of judgment; (4) the time of a judgment varies with individuals and with all the circumstances which are likely to influence the subject's degree of confidence." These measurements are admittedly rough and the authors suggest that it might be worth while in the future to undertake the more exact time measurements in psychophysical experiments where this would not interfere with the main purposes of the investigation.

Daily experience reveals great variations in the length of time taken for pronouncing judgments under different conditions and by different individuals and suggests that the time of a judgment is in some way correlated with its accuracy. There would supposedly be a considerable difference in both the character and accuracy of a judgment rendered after half a second and one given after one, two or three seconds. One might expect that, eliminating adaptation and fatigue, the judgment would grow in accuracy the longer the time taken to make it, provided the stimuli remained until the judgment was rendered. There would be a gradual maturing of the factors involved and a corresponding increase in confidence and accuracy. In many cases this does represent the course of a sensory judgment. The judgment of difference grows steadily to a point of maximal clearness. However, one notes introspectively that in many cases a prolongation of the time beyond that required for the first judgment that could be made introduces doubt and indecision. Instead of

an increase in certainty and accuracy there is an increase in uncertainty and inaccuracy. Both considerations suggest that there may be an *optimal* time for judgment, varying with individuals and with varying stimuli and conditions. If this be true then wrong judgments would include among others those given before the optimal time had been reached or after it.

Introspection reveals various alternative processes, even in the short times required to judge the lengths of the two lines when the differences are liminal: (1) Prepossession leads one to expect a line to the right or to the left and a mere glance from one line to the other gives a very quick and coercive judgment, that is given with confidence and is reliable. (2) An initial tentative judgment is gradually confirmed and there results a steady increase in confidence in its accuracy. In such cases the difference between the lines appears to grow before one's eyes and the judgment is finally made with confidence and accuracy. Such judgments are relatively quick. (3) There may be a rapid alternation of judgments beginning either with an initial judgment of equality or with a tentative judgment of difference, with a change of the judgment as each line is successively fixated or attended to. Here there are in a single experiment lasting a second or less not one but several judgments, the final judgment being a compromise, taking usually a longer time than judgments of the other types and ordinarily given with little confidence and reliability. The resolution of an initial judgment of equality sometimes takes place with a sudden expansion or telescoping of one line or the other, or again it may take place slowly. (4) The first tentative judgment is gradually or quickly followed by doubt and finally by reversal, sometimes with great confidence and reliability, but more often with little confidence and reliability. All of these instances are typical cases which suggest the possible significance of the time factor in judgment.

It was with a view of studying the relation between the times of judgments and their accuracy, as well as the determination of individual differences in judgment times, that

the present investigation was undertaken. The stimuli employed were lines, chosen because of their convenience for exposure and time measurements. The lines were 20 and 20.3 mm. long, 2 mm. in width, and printed horizontally on the cards 10 mm. apart. The length of lines, which differed by 1/66.6, was selected with a view to securing a stimulus difference that would give approximately 84 per cent. of right cases, in accordance with Fechner's suggestion that such a difference would give the most satisfactory results. The plate from which the cards were printed was prepared by an expert engraver and the lines were very accurate. The cards were exposed by means of the spring drop screen used by the writer in experiments on the time of perception as a measure of differences in sensations.[1] The exposure was continued until the reaction or judgment was made. Immediately after the reaction the shutter was closed. The average of a great many trials showed that the screen was raised approximately $300\sigma$ after the reaction. In one half of the judgments the subjects were to judge which was the longer line and in the other half the shorter. The times were recorded in the usual manner with the Hipp chronoscope, reactions being made by the right hand if the line to be judged was to the right and by the left hand if it was on the left. The distances of the subjects' eyes from the screen was 50 cm.

From each of three subjects, Miss Madge Brown (Br), of the University of Colorado, to whom I am under especial obligations for great assistance both in the conduct of the experiments and in the laborious calculations, Mr. C. L. Bluemel (Bl), also of the University of Colorado, and the writer (H), 1,000 judgments and judgment times were secured. Fifty judgments constituted a series and ordinarily one series a day was taken. The usual precautions were observed to secure uniformity in conditions, such as control of the chronoscope, time of day, order of series, uniformity in light conditions, etc. Immediately after each judgment the subjects were asked to assign the degree of confidence in the accuracy

[1] For description see writer's 'The Time of Perception as a Measure of Differences in Sensations,' *Archives of Philos., Psychol. and Scientific Methods*, Science Press, New York, 1906, p. 18.

of the judgment, *a* for 'perfectly confident,' *b* 'fairly confident,' *c* 'with little confidence' and *d* 'doubtful.' In each case the subject was asked to judge whether a line was longer or shorter hence the small number of the 'equal' and 'undecided,' cases which occurred are included under the *d* judgments.

The general summary of the results of 3,000 judgments appears in Table I., which gives the distribution of the right and wrong cases with each degree of confidence, the corresponding judgment times in $\sigma$ or thousandths of a second, and the mean variations. The last line in the table gives the total number of right and wrong cases and the average time of judgment.

The accuracy of judgment is shown by the percentages of right cases, which are 82 per cent., 77.4 per cent. and 83.2 per cent. If the *d* judgments are divided equally between the right and wrong cases, according to the usual procedure, these percentages become 82.1 per cent., 73.2 per cent. and 81.6 per cent. However, the *d* judgments, at least for subjects Br and H, include cases which are not strictly doubtful or equal, but are rather judgments with a minimum of confidence.

The correspondence between confidence and accuracy is high, indicating the reliability of the introspections. There is a uniform decrease in the number of right cases with the decrease in confidence as the following percentages of right cases with each degree of confidence show:

| | *a* | *b* | *c* | *d* |
|---|---|---|---|---|
| Bl. | 96.6 | 84.1 | 65.3 | 48.0 |
| Br. | 95.1 | 94.4 | 83.6 | 61.5 |
| H | 100. | 97.2 | 85.3 | 56.3 |
| Av. | 97.2 | 91.9 | 78.1 | 55.3 |

The nature of the decrease in accuracy with decrease in confidence and the correlation between the subjective scale of confidence and the degree of mathematical probability is a problem which is beside the purpose of this study.[1]

[1] Cf. Fullerton and Cattell, 'Perception of Small Differences,' Philadelphia, 1892, pp. 14, 60–64, 124–127, 132–133, 142–145.

## TABLE I

| Degree of Confidence | Bl | | | | | | Br | | | | | | H | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | R. | | | W. | | | R. | | | W. | | | R. | | | W. | | |
| | No. | σ | M.V. | No. | σ | M.V. | No. | σ | M.V. | No. | σ | M.V. | No. | σ | M.V. | No. | σ | M.V. |
| *a*..... | 337 | 753.1 | 190 | 12 | 557.0 | 132 | 98 | 559.5 | 136 | 5 | 574.4 | 148 | 93 | 637.6 | 104 | 0 | | |
| *b*..... | 318 | 1044.8 | 308 | 60 | 986.9 | 317 | 235 | 595.9 | 165 | 14 | 668.9 | 206 | 312 | 721.8 | 124 | 9 | 698.8 | 122 |
| *c*..... | 128 | 1311.3 | 356 | 68 | 1204.7 | 344 | 216 | 635.2 | 151 | 66 | 605.9 | 171 | 285 | 788.7 | 133 | 49 | 777.3 | 143 |
| *d*..... | 37 | 1612.0 | 519 | 40 | 1499.1 | 555 | 225 | 624.0 | 156 | 141 | 596.2 | 150 | 142 | 849.6 | 145 | 110 | 813.6 | 147 |
| | 820 | 992.1 | | 180 | 1154.3 | | 774 | 610.4 | | 226 | 602.7 | | 832 | 770.3 | | 168 | 796.8 | |

The subjective scale at any rate is less 'crude and inconstant' than Titchener apparently holds it to be and the correlation is relatively high.[1] His statement that "We 'feel sure,' our expectation becomes conviction, long before we have the objective right to be anything more than very moderately expectant" is not borne out by these experiment. It is perhaps worth noting that the percentage of right *d* judgments (55 per cent.) is less than that obtained by Pierce and Jastrow (60 per cent.), by Fullerton and Cattell (60 per cent. and 65 per cent.) and by Griffing (59 per cent.). The individual differences both in the degree of confidence and in the times of judgment are discussed in a later section.

The times of judgment for all right judgments is shorter than the average for the wrong judgments, except in the case of Br, where the difference is not significant. However, in all cases but two the times for wrong judgments under each category of judgment is longer than for the right judgments. The times of wrong judgments are both absolutely and relatively more variable.

The relation between the time of judgment and degree of confidence is indicated by a uniform increase in time with a decrease in confidence, except in the case of the *c* and *d* judgments of Br. Martin and Müller[2] suggest that "if the cases in which the judgment kl or gr are given be arranged in classes according to the times of judgment, in which the lowest class would include the longest and the highest class the shortest judgment times then perhaps the distinction between kl [less] and *kl* [less (clearly)], gr [greater] and *gr* [greater (clearly)] cases might be dispensed with altogether." With small differences where the absolute impression does not enter in as a factor and where the variability in time of judgment is as great as is these experiments, this could certainly not be done. The rise of judgments to the same degree of clearness, so far as one can introspectively estimate it, may be quick or slow, even though the average times do show a definite correspondence between time and confidence,

[1] Titchener, E. B., 'Experimental Psychology, Quantitative, Students Manual,' New York, 1905, pp. 53–55.

[2] *Op. cit.*, p. 206.

The uniformity in the increase in time is indicative of the absence of such factors as the absolute impression, slip comparisons, etc., which would be likely to betray themselves in lengthened or shortened judgment times.

The nature of the correlation between the time and accuracy of judgment is difficult to determine and indicate. Müller[1] gives the psychological conditions of *zufällige Fehlervorgänge* as "(*a*) Varying degree and direction of attention; (*b*) variation of the constituent process in the given complex by which judgment is determined; (*c*) degree and direction of expectation and imagination; (*d*) influence of chance order of preceding experiments" (quoted from Titchener, 'Experimental Psychology, Quantitative, Instructor's Manual,' p. 313). The introspective records kept in the course of these experiments suggested that among the conditions that lead to wrong judgments, the time factor is significant. The subjects noted in many cases that the judgments, which later were shown to be wrong, included a considerable number that were judged to have been made either too soon or too late. This is indicated to some extent in Table II, which gives the distribution of the right and wrong cases according to time and the percentage of cases falling within such time-groups.

Owing to the complexity of the factors that cause erroneous judgments the evidence for the existence of the two types of wrong judgments does not emerge as clearly as the introspections led us to expect. H shows a greater percentage of long erroneous judgments, Br a greater percentage of short erroneous judgments, while Bl gives slight evidence of both types. The differences in percentages are small and are obscured by a variety of other factors. A still larger number of measurements would be required to indicate the time factor decisively and in addition a series of experiments where the time taken in judgment was voluntarily controlled.

One curious constant error in judgments of the shorter line appeared in the results. All of the subjects, particularly

[1] Müller, G. E., 'Die Gesichtspunkte und die Tatsachen der psychophysischen Methodik,' Wiesbaden, 1904, § 21.

## TABLE II

| | | 100–300 | 300–500 | 500–700 | 700–900 | 900–1100 | 1100–1300 | 1300–1500 | 1500–1700 | 1700–1900 | 1900–2100 | 2100–2300 | 2300–2500 | 2500+ |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Bl | Right cases | 1 | 46 | 185 | 197 | 135 | 84 | 64 | 50 | 26 | 14 | 10 | 4 | 4 |
| | Wrong cases | 1 | 11 | 29 | 23 | 24 | 29 | 21 | 24 | 6 | 4 | 1 | 4 | 3 |
| | Per cent. R. | .001 | .056 | .225 | .240 | .165 | .102 | .079 | .061 | .032 | .017 | .012 | .005 | .005 |
| | Per cent. W. | .006 | .061 | .161 | .127 | .133 | .161 | .116 | .134 | .033 | .022 | .006 | .023 | .017 |
| Br | Right cases | 2 | 261 | 302 | 133 | 52 | 18 | 5 | 1 | | | | | |
| | Wrong cases | | 89 | 79 | 37 | 13 | 8 | | | | | | | |
| | Per cent. R. | .003 | .337 | .390 | .174 | .066 | .023 | .006 | .001 | | | | | |
| | Per cent. W. | | .394 | .350 | .164 | .057 | .035 | | | | | | | |
| H | Right cases | 3 | 36 | 286 | 350 | 127 | 25 | 5 | | | | | | |
| | Wrong cases | | 7 | 47 | 69 | 33 | 11 | 1 | | | | | | |
| | Per cent. R. | .004 | .043 | .344 | .421 | .152 | .030 | .006 | | | | | | |
| | Per cent. W. | | .042 | .279 | .410 | .197 | .066 | .006 | | | | | | |
| Av. | Right cases | 6 | 343 | 773 | 680 | 314 | 127 | 74 | 51 | 26 | 14 | 10 | 4 | 4 |
| | Wrong cases | 1 | 107 | 155 | 129 | 70 | 48 | 22 | 24 | 6 | 4 | 1 | 4 | 3 |
| | Per cent. R. | .001 | .145 | .320 | .278 | .128 | .052 | .030 | .021 | .011 | .006 | .004 | .001 | .002 |
| | Per cent. W. | .006 | .166 | .263 | .233 | .129 | .087 | .041 | .045 | .011 | .007 | .002 | .008 | .006 |

Br and H, noted early in the experiments that judgment could be given more easily, more quickly and with greater confidence when reaction was to be made to the shorter line. The feeling that the most accurate judgments would be secured with the shorter line was very marked. Table III gives the distribution of judgments to the longer and shorter line.

The results in part confirm the introspections and in part do not. The general averages show in each case that the greater number of wrong judgments was obtained from reactions to the shorter line though the differences are significant only in the case of Br. However, the number of right *a* judgments to the shorter line is almost twice as great as to the longer line, except in the case of Bl where the difference is not marked. The results indicate in an interesting way both the accuracy and inaccuracy of introspections in the judgment of small differences.

In summary the following conclusions with reference to the relation of the time of judgment to accuracy and confidence may be noted:

1. The time of judgment increases uniformly as the degree of confidence decreases.

2. The time of wrong judgments is on the average longer than the time of right judgments, while under each category of judgment the wrong judgments are in general shorter.

3. The time of wrong judgments is more variable than that of right judgments, and there are indications of two types of wrong judgments with reference to time, those that are rendered too quickly and those that are prolonged beyond the optimal time.

A continuation of this investigation (1) where the time of exposure of stimuli was limited, (2) where the time of judgment was voluntarily shortened or prolonged, (3) and with varying differences in stimuli, should give significant results.[1]

[1]The writer had planned such an investigation, but a change of work has necessitated its indefinite postponement; hence the publication of these preliminary results.

## TABLE III

| | Bl | | | | Br | | | | H | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | R. | | W. | | R. | | W. | | R. | | W. | |
| | Longer | Shorter | Longer | Shorter | Longer | Shorter | Longer | Shorter | Longer | Shorter | Longer | Shorter |
| *a*..... | 160 | 177 | 2 | 10 | 33 | 65 | 1 | 4 | 33 | 60 | 0 | 0 |
| *b*..... | 178 | 140 | 29 | 31 | 126 | 109 | 6 | 8 | 149 | 163 | 2 | 7 |
| *c*..... | 62 | 66 | 40 | 28 | 128 | 88 | 27 | 39 | 155 | 130 | 18 | 31 |
| *d*..... | 14 | 23 | 15 | 25 | 114 | 111 | 66 | 75 | 81 | 61 | 62 | 48 |
| Total..... | 414 | 406 | 86 | 94 | 401 | 373 | 100 | 126 | 418 | 414 | 82 | 86 |

## Individual Differences

Experiences in daily life show the wide range of differences in the speed of judgment as well as in accuracy and the degree of confidence with which it is made. With a view to securing data on these differences and their correlations, 500 judgments and judgment times, under the same conditions and with the same stimuli as used in the above described experiments, were obtained from each of seven subjects, five women and two men, all of whom had had practically a year's laboratory training in psychology or more. These measurements, together with those of the three subjects of the above experiments, thus give records for six women and four men. The results appear in Table IV.

What strikes one at first is the variation in the times of judgments ranging from averages for all judgments of $304\sigma$ for A to $1095\sigma$ for E. The group is too small to reveal type differences which from common observation might be expected to occur. The variation in accuracy runs from 68.6 per cent. of right cases for A to 83.2 per cent. for H, the range being almost exactly as 1:2. This is the variability noted by Cattell[1] in the judgments of the ten observers who ranked the American psychologists and which he says is "approximately the variability that I have found in normal individuals in other mental traits, accuracy of perception, time of mental processes, memory, etc." Wells[2] found a similar variability in judgments of literary merit. With reference to sex differences in rapidity of perception and sense judgments it is generally supposed that women are quicker than men. This difference does not appear in these results. Subjects A, B, C, D, E and Br are women; F, G, Bl and H are men. The shortest judgment times are by two women, A and B, and the two longest times are also by two women, D and E. In accuracy the sex differences are insignificant, the averages for the men being 77.2 per cent. and for the women 76.9 per cent., with the two highest percentages by two men, Bl and H.

[1] Cattell, J. McKeen, 'Statistics of American Psychologists,' *Am. Jour. of Psychol.*, XIV., 1903, p. 581.

[2] Wells, F. Lyman, 'A Statistical Study of Literary Merit,' *Archives of Psychology*, New York, 1907, p. 27.

## Table IV

| | A | | | | | | B | | | | | | C | | | | | | D | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Right | | | Wrong | | | Right | | | Wrong | | | Right | | | Wrong | | | Right | | | Wrong | | |
| | No. | Time | M.V | No. | Time | M.V | No. | Time | M.V | No. | Time | M.V | No. | Time | M.V | No. | Time | M.V | No. | Time | M.V | No. | Time | M.V |
| *a* | 58 | 350 | 102 | 7 | 427 | 120 | 304 | 518 | 129 | 38 | 513 | 150 | 154 | 513 | 87 | 11 | 443 | 59 | 376 | 1031 | 373 | 76 | 1006 | 347 |
| *b* | 152 | 299 | 86 | 18 | 320 | 95 | 79 | 599 | 188 | 62 | 527 | 133 | 153 | 657 | 129 | 18 | 559 | 122 | 21 | 1242 | 664 | 20 | 1215 | 586 |
| *c* | 76 | 281 | 64 | 28 | 296 | 94 | 6 | 734 | 142 | 11 | 608 | 222 | 72 | 794 | 159 | 45 | 651 | 114 | 2 | 1162 | 334 | 4 | 963 | 427 |
| *d* | 57 | 292 | 83 | 104 | 289 | 82 | | | | | | | 21 | 914 | 214 | 26 | 687 | 132 | | | | 1 | 1043 | |
| Total R. and W. cases and av. times | 343 | 305 | | 157 | 300 | | 389 | 536 | | 111 | 530 | | 400 | 652 | | 100 | 621 | | 399 | 1043 | | 101 | 1046 | |
| Av. R. and W. | 304 | | | | | | 535 | | | | | | 646 | | | | | | 1044 | | | | | |

| | E | | | | | | F | | | | | | G | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Right | | | Wrong | | | Right | | | Wrong | | | Right | | | Wrong | | |
| | No. | Time | M.V | No. | Time | M.V | No. | Time | M.V | No. | Time | M.V | No. | Time | M.V | No. | Time | M.V |
| *a* | 287 | 1008 | 270 | 52 | 1153 | 374 | 287 | 592 | 138 | 50 | 522 | 125 | 122 | 809 | 177 | 8 | 843 | 208 |
| *b* | 77 | 1175 | 272 | 44 | 1409 | 482 | 40 | 643 | 144 | 51 | 531 | 155 | 141 | 919 | 171 | 30 | 909 | 155 |
| *c* | 14 | 1161 | 265 | 12 | 1347 | 453 | 15 | 734 | 166 | 19 | 567 | 153 | 105 | 968 | 183 | 83 | 947 | 209 |
| *d* | 11 | 986 | 395 | 3 | 1233 | 113 | 6 | 717 | 148 | 32 | 525 | 130 | 3 | 1203 | 344 | 8 | 1050 | 200 |
| Total R. and W. cases and av. times | 389 | 1046 | | 111 | 1268 | | 348 | 606 | | 152 | 531 | | 371 | 899 | | 129 | 938 | |
| Av. R. and W. | 1095 | | | | | | 583 | | | | | | 909 | | | | | |

Interesting differences in the use of the four categories of confidence occur. The frequency with which each degree of confidence is employed and the percentage of right cases are indicated in the following table:

TABLE V

PERCENTAGE OF TIME EACH DEGREE OF CONFIDENCE IS USED

| | A | B | C | D | E | F | G | Bl | Br | H | Av |
|---|---|---|---|---|---|---|---|---|---|---|---|
| *a* | 13.0 | 68.4 | 33.0 | 90.4 | 67.8 | 67.4 | 26.0 | 34.9 | 10.3 | 9.3 | 42.0 |
| *b* | 34.0 | 28.2 | 34.2 | 8.2 | 24.2 | 18.2 | 34.2 | 37.8 | 24.9 | 32.1 | 27.6 |
| *c* | 20.8 | 3.4 | 23.4 | 1.2 | 5.2 | 6.8 | 37.6 | 19.6 | 28.2 | 33.4 | 18.0 |
| *d* | 32.2 | | 9.4 | .2 | 2.8 | 7.6 | 2.2 | 7.7 | 36.6 | 25.2 | 12.4 |
| Per cent. of right cases | 68.6 | 77.8 | 80.0 | 79.8 | 77.8 | 69.6 | 74.2 | 82.0 | 77.4 | 83.2 | |

It is evident that in spite of instructions the various degrees of confidence come in the course of the experiment to have very different values. Some subjects are nearly always perfectly confident, while others are very cautious, *e. g.*, *a* was used 90.4 per cent. of the time by D and 9.3 per cent. by H. Subject B gave no *d* judgments at all and subjects B, D, E and F rarely gave either *c* or *d* judgments. This is an indication of the great difficulty of assigning the degrees of confidence in such measurements, particularly by untrained observers. The averages for the men and women separately show that the women used *a* 47.2 per cent. of the time and the men 34.4 per cent., but the variability is too great to make the difference a significant one.

The correlation between the degree of confidence and accuracy is indicated in Table VI.

TABLE VI

PERCENTAGE OF TIME SUBJECTS WERE RIGHT WITH EACH DEGREE OF CONFIDENCE

| | A | B | C | D | E | F | G | Bl | Br | H | Av. |
|---|---|---|---|---|---|---|---|---|---|---|---|
| *a* | 89 | 89 | 93 | 83 | 85 | 85 | 94 | 97 | 95 | 100 | 91 |
| *b* | 89 | 56 | 89 | 51 | 64 | 44 | 83 | 84 | 94 | 97 | 75 |
| *c* | 73 | 35 | 62 | 33 | 54 | 44 | 56 | 65 | 84 | 85 | 59 |
| *d* | 35 | | 45 | | 79 | 16 | 27 | 48 | 62 | 56 | 41 |

While the percentages of right cases decreases with the decrease in confidence the degree of confidence is no very accurate measure of the reliability of the judgments. Thus

subject D who was perfectly confident in 90.4 per cent. of the judgments and was right in but 83.1 per cent. of them was nevertheless above the average in accuracy. The relation of confidence to accuracy seems to be an individual matter without any well-defined general tendency. It is impossible, for instance, to say that those subjects who are most confident are either more accurate or less accurate than those who are cautious for the latter may either be very accurate or inaccurate. Fullerton and Cattell[1] found that those observers who had the greatest confidence had the largest probable errors. Griffing,[2] however, found no correlation between confidence and the probable error. No significant sex differences in relation of confidence to accuracy are shown.

A significant correlation does not appear between the time of judgment and its accuracy. Those whose judgments are given quickly are neither more nor less accurate than those whose times are longer, as the following table shows:

TABLE VII

| | A | F | G | Br | B | E | D | C | Bl | H |
|---|---|---|---|---|---|---|---|---|---|---|
| Per cent. of right cases | 68.6 | 69.6 | 74.2 | 77.4 | 77.8 | 77.8 | 79.8 | 80.0 | 82.0 | 83.2 |
| Times | 304 | 583 | 909 | 608 | 535 | 1095 | 1044 | 646 | 1021 | 753 |

The three most accurate subjects, C, Bl and H, are distributed among those whose times would be short, intermediate and long.

Summarizing the results of the experiments on individual differences the following conclusions are noted:

1. The range of variability in accuracy in discriminating lengths of lines, as determined by these experiments, is as 2:1.

2. The time of judgment varies from one third of a second to one second.

3. The degree of confidence ranges from subjects who are perfectly confident in 90 per cent. of 500 judgments to those who are perfectly confident in less than 10 per cent.

4. While there is a positive correlation on the whole be-

[1] *Op. cit.*, p. 126.

[2] Griffing, H., 'On Sensations from Pressure and Impact,' PSYCH. REV. MON. SUPPLEMENTS, 1895, p. 46.

tween degree of confidence and accuracy the degree of confidence is not a reliable index of accuracy.

5. Subjects whose judgments are given most quickly are neither more accurate nor less accurate than those whose judgments are longer.

6. There are no significant sex differences in accuracy in discriminating lines, nor in time of judgments, nor in confidence, nor in the correlations of time, accuracy and degree of confidence.

# EXPERIMENTAL STUDIES OF RHYTHM AND TIME

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## II. THE PREFERRED LENGTH OF INTERVAL (TEMPO)[1]

### A. THE PREFERRED TEMPO AS DETERMINED BY THE METHOD OF PAIRED COMPARISON OF METRONOME CLICKS

The question as to what particular tempo, or length of interval between auditory impressions, is felt to be the most pleasing, or the most favorable for rhythmizing sounds, has been investigated by various methods, and conclusions have been reached which have not always been in complete accord. It seemed to the writer that the problem ought to be investigated afresh by means of different methods.

In the first investigation recorded here a method of paired comparison of the beats of a metronome (Verdin's make) was employed. The notches on the pendulum of the instrument were so arranged that the following speeds could be obtained by properly sliding the pendulum weight: 40, 48, 56, 63, 72, 80, 88, 92, 96, 100, 104, 108, 112, 116, 120, 124, 128, 132, 136, 140, 144, 148, 152, 156, 160, 168, 176, 184, 192, 200 and 208. A few times, as the rates of the clicks became nearly equal, the weight was moved half way between the notches, so as to halve the difference between the two rates. The half-way point was not determined precisely, but the resulting errors are probably negligible.

[1] An earlier article in this series on 'Qualitative Limens or Grades of Rhythm, and the Difference Limens in the Perception of Time,' appeared in the PSYCHOLOGICAL REVIEW for March, 1911. The remaining articles will detail the results of experiments on the estimation of the mid-points between pairs of different tempos by two methods, and the grouping of metronome clicks into the maximum number of speed categories. Some of the topics discussed here will come up again in these later articles. The metronome experiments described in this article were made in 1905; the observations of the rhythmical responses in the theaters were made from time to time during 1905 and 1906.

The accuracy of the metronome was determined by means of the Hipp chronoscope and a 100-DV fork, particularly by means of the latter. A Deprez marker, electrically connected with the metronome, recorded the contacts made by the swings of the pendulum on the smoked drum of a kymograph along the time line. The metronome was allowed to click off about a dozen beats before the measurements were recorded. It was found that all the rates measured which were faster than 60 per minute were slightly too slow and that the required correction differed somewhat for different tempos. The 1.50 sec. interval (40 beats per minute) was too *fast* by .13 sec., and the 1 sec. interval by .008 sec. The following intervals were too *slow* by the amount indicated: .909 sec. (66 per minute), by .003 sec.; .833 sec. (72 per minute), by .016 sec.; .682 sec. (88 per minute), by .023 sec.; .625 sec., by .013 sec.; .50 sec., by .032 sec.: .416 sec., by .012 sec.; .375 sec., by .011 sec.; .340 sec., by .16 sec.; .320 sec., by .015 sec.; and .288 sec., by .017 sec. (These are all fork measurements, except for .416 and .625.) No measurements were made for some of the speeds selected as the most favorable; in such cases the correction found empirically for the nearest rate was used.[1]

The 'make' and the 'break' checks showed considerable regularity when each was considered separately, but the make checks were closer together than the break, except in two cases, where they were .12 sec. and .024 sec. slower (for the 1.37 and .912 speeds). The differences were as follows: .007 sec. (rate, 60 per minute), .007 (72 per minute), .012 (88 per minute), .004 (120 per minute), .027 (160 per minute), .042 (184 per minute), and .033 sec.[2] (208 per minute). The swings of the pendulum in the two directions were not therefore precisely equal; but the irregularity apparently had little

[1] In 1906 measurements were made by the fork of the rates 80, 96, 104, 112, 124, 126 and 144 (beats per minute). Each interval was found to be too slow by the amount indicated in parenthesis: .75 sec. (by .025 sec.); .625 (.021); .577 (.032); .535 (.026); .484 (.023); .476 (.033); and .416 (.25). By consulting the table it will be noticed that the correction used for .577 was .009 sec. smaller than the empirical result, and .013 too small for .416 sec. These errors are immaterial for the purpose in hand.

[2] In the 1906 measurements the 'break' records were likewise shorter by about the same amounts, except for the speeds (corrected readings) .507 and .775 sec.

influence on the subjects' judgments, for, according to the introspections, the beats were heard as regular, except for Ho for the faster speeds, by H once in the middle of a series, by I once with a slow speed, by K once or twice, and by Wa, J, E, Ea and Z, who at times heard the first few beats as irregular.

Twenty subjects served in the investigation, which was carried out at Princeton, 18 being second-year students in experimental psychology (seniors), one a graduate student in psychology and one a professor of psychology (Professor Warren).

The procedure was as follows: With their backs toward the instrument the subjects listened to pairs of different speeds of clicks, about a dozen clicks for each speed. The initial pair compared always consisted of the extreme rates, 208 and 40. To one half the 208 rate was presented first; to the other half, the reverse. The order was changed during the successive comparisons, so that during half the time the first series consisted of the faster rate and the second series of the slower rate. After having listened to the initial pair, the subjects were asked to state which tempo was preferred, or which was felt to be the more agreeable.

The next step was to compare the selected or preferred tempo with the speed adjacent to the non-preferred tempo. To illustrate: suppose that 208 was preferred to 40; 208 was then compared with 48, then, if still preferred, with 56, and so on until some slower rate was selected. The slower rate was then, in turn, compared with 200; then if preferred, with 192, etc. Each preferred rate was repeated until preference was expressed for the other member of the pair. In this way the speeds, by a series of successive approachments, grew more and more alike until finally the rates were objectively equal.

Here it is of interest to state that the point of equality was sometimes anticipated. Speeds which were objectively different were pronounced equal 16 times by 12 subjects. In half of these mistaken judgments the speeds differed by two beats per minute (the slowest pair confused being 63 and

65 and the fastest 198 and 200); in seven, by 4 beats (the extreme rates confused being 96 and 100 and 204 and 208); and in one case by 12 beats.[1] It will be observed that in all of these instances the differences in the time between the rates are decidedly above the limits of the ability to discriminate different durations of sounds, which according to Helmholtz is 1/132 and according to Exner 1/500 sec.; and that they are also larger than the threshold of difference for empty intervals. Hall and Jastrow found that a variation of 1/60 of empty intervals between clicks 4.27 sec. apart was always correctly noticed, while one of 1/120 was nearly always noticed. Our differences were from 1/30 to 1/5 sec. In the case of these confused judgments there could, in theory if not in practice, be no choice between the rates (except possibly in three cases in which the mistakes were rectified). In all cases, however, the steps were continued until the speeds became objectively equal.

After the speeds had become objectively equal, the subjects listened to about five pairs of the same speeds (without being told that they were equal), and reported their observations respecting the rates and the preference. The introspections showed that the rates were judged equal 37 times, unequal 21 times and doubtful 5 times. The rates were judged to be faster in the first series 8 times as against 14 times in the second series, indicating that the last or most frequent of series of equal sounds appear to be faster. The last series was preferred 16 times, the first 33 times, and no-preference judgments were made 28 times. It is therefore apparent that when the rates of clicks are objectively equal, the tendency is about twice as strong to prefer the first series as the second. That is, the rate which is subjectively slower is more agreeable. There were some subjects, however, who always preferred the last series, some always the first, and some changed their preferences.

These preferences while the rates were objectively equal may be compared with the relative number of preferences

[1] Twice a rate which was two beats slower than the other was called faster, and once when the difference was 8 per minute. The faster rate was judged slower five times, with the rates differing by 2, 4, 12 and 16 per minute (the latter three for I.)

for the two series before the rates became equal. The first was preferred in the gross 309 times, and the second 318. Here the tendency is in the opposite direction, but the difference is so small as to be practically negligible, as was to be expected from the fact that the time order was alternately changed. There were six subjects, however, in which the order of sequence had a considerable influence on the results. Three preferred the first series of beats about twice as frequently as the second (Ho, McD, Wa), and three just the reverse (K, J, E). With nine the number of times the first was preferred more frequently than the second, or vice versa, did not exceed three times each (C, T, L, W, H, Co, Ea, Z, Wy). While it thus appears that the constant time errors can be ruled out for the majority by alternating the series, for a considerable number this appears not to be the case.

After having listened to the same speeds, as just described, the subjects were, finally, asked to give close attention while two longer series of the same speeds were clicked off, and then to state whether they were entirely satisfied with the preferred interval they had selected. The results given in the tables (Tables I. and II.) represent these final preferences. It was found that the great majority (sixteen) were satisfied with the selections they had made, and that only four changed their preferences: three to a slightly slower speed and one to a faster one. This seems to show that the preferences were quite decided. Four stated that it was easy to arrive at a decision, two that it was usually easy, and two that it was sometimes difficult. That there were difficulties involved in making the comparisons, of which the subjects may not always have been clearly conscious, will be seen presently.

To arrive at the final choice, it was found that the subjects were obliged to change their preference on an average of 8.5 times. But the individual differences in this respect were quite considerable. Five changed less than four times (Mo, T, L, E and Z), while seven changed more than ten times (M, C, B, H, Wa, J, E).

At what point in listening to the second series of clicks was the preference made? According to the introspections,

the decision is most frequently reached in the early part of the second series. Nine so reported (one did so usually, and another did so except when the rates were close together), while three made the choice in the middle of the series and five toward the close or after the close of the series. The promptness in deciding on the preferences would seem to indicate that the judgments were largely of the *immediate type*. The subjects were asked whether their judgments were immediate; that is, based directly upon the sensory impressions, or whether they were of the reflective type. Seventeen considered that they were based directly upon the sensations, or upon the immediate effects of the excitations; and five stated that they had to reflect more or less to arrive at a choice (one between the series concerning the first series, and two when the rates approached equality).

If we conclude that the comparison judgments and choices were of the immediate type, are we then justified in inferring that the first series of sounds remained in the mind during the second or part of the second series, as after sensations or primary memories? On this point ten reported that they could easily carry or retain the first speed, two could do so fairly well, and two found it possible to ideate or image the earlier tempos. But a considerable majority—even those who said they retained the first series well—resorted to various expedients by means of which to retain or reinvisage the first tempo. Ten tapped (the first series) with the fingers, some quite frequently, some only when the rates were nearly equal, and nearly all did so with the express purpose of keeping the first series in mind (M, B, J, C, Ea, Z, Ko, Wy, I and T). Movements of the foot (by W, C and Ko) and of the head (by Mo; W, who felt his head swaying, and L, who moved unconsciously) were reported less often, three times each. One used certain muscular strains or tensions for the purpose of retaining the first rate (Wa), and one visualized a line in space for the same purpose (E). While four, on enquiry, stated that they did not vocalize the beats (W, I, H and Wa), three did (L, as 'drum, drum,' and Ko and Wy, as 'tick-tack'), and one believed that he did. This vocalization may

have been merely a tendency toward rhythmical expression, rather than an effort to reinvisage the earlier tempo. Apparently these facts warrant the conclusion that the first tempo was usually not directly (or unaided) compared with the second. The primary memories of the first series were often so vague that some means had to be invoked to revive the sensations. These means were nearly always *motor* in character. The movements or strains used served as substitutes for the first series. Vocalization would have aroused sensations of the same modality as the sensations compared, and would therefore, presumably, have proved a hindrance rather than an aid. The comparisons were then, at times, simultaneous rather than successive. The second tone series was compared with a substitute series of sensations (motor) going on at the same time.

The further consideration of the results may conveniently be made in connection with the following numbered conclusions:

1. We find a very general tendency to subjectively rhythmize periodic auditory impressions of the same intensity, provided the rates are not too slow or too fast. Only two of our subjects failed to rhythmize the beats (McD and H). Two got no rhythm at the beginning of the series (J, T); one got none when he tried to base his judgments on 'pure sensation'; two got none when the rates were very slow or very fast (L, Co); another when they were very slow (T); and one did not get any rhythm for 'some speeds.' One experienced rhythm a few times only, with the slow beats. Three got rhythms with the slow beats more readily than with the fast (Mo, M, Co), and two the reverse (Ho, Z). One asserted that he apprehended all rates as rhythmical. Within the limits of 208 and 40 sounds per minute there is thus no uniform or very clearly marked tendency. Five reported that they perceived rhythms although the beats were apprehended as equally loud, or without accent (Ko, R, Ea, Wa; Co, sometimes; Wa visualized the measures in the form of two long strokes, ——).

The dominant pattern was the trochee (— ⏑). This was

obtained by nine subjects (Ho, K, Co, Mo, T, L, J, Z and Wy). The iambic was reported by five (M; Ho, sometimes with the faster rates; B, with all rates; Co, and I. I, however, was not certain). C obtained ⏑ ⏑ — with very slow rates.

In order to ascertain the basis of the subjective rhythms, some of the observers were asked whether they noticed any qualitative difference in the tones from the two swings of the metronome pendulum for any given rate. Three felt that there was a slight difference, without being able to designate what it was (B, H and L); one perceived a difference in loudness (M); one in resonance (Wa); one heard one beat as more muffled (W); four remarked a pitch difference (I; J; H, especially with the slow rates; Co heard the second series with slow rates in a higher pitch, and the first series with fast rates); one "thought of the click on the left side as suspended and on the right side as finished"; and two did not perceive any difference. These qualitative differences were undoubtedly at the basis, at least in part, of the felt subjective rhythm. The pitch element of emphasis, surprisingly, plays the dominant rôle. It has been shown elsewhere that pitch may substitute for the elements of loudness or duration in speech centroids.[1] The metronome used is not now available, and I cannot say whether there was any objective pitch difference in the two clicks. Such differences, if they existed, were probably very small, since many subjects made no mention of them.

The main results will be found in accord with Bolton's findings; he found the same general tendency toward subjective rhythmization and preponderance of the trochee pattern.

2. It is the exception rather than the rule that different interval lengths between objectively equal sounds are felt as indifferent or neutral. One of two contrasted rates or tempos is usually preferred to the other. There were only 17 instances by ten subjects in 700 or more comparisons in which no preference was expressed. This is no doubt

[1] Wallin, 'Researches on the Rhythm of Speech,' Studies from the Yale Psychological Laboratory, 1901, IX., pp. 9–23.

partly due to the fact that the task confronting the subjects was the expression of a choice, although the subjects were not told that no-preference judgments were barred. Granting the force of this criticism, the conclusion will probably hold, that different rates of auditory impressions tend to arouse favorable or unfavorable attitudes in the listener. An analysis of the records enables us to formulate the following rule regarding neutral and doubtful judgments: intervals tend to arouse an indifferent judgment either when they are barely different or when they are very noticeably different; but, while most of the *neutral* reactions are obtained when the rates are nearly alike, most of the *doubtful* judgments are rendered when the rates are very different. Thus there were ten no-preference judgments rendered when the difference between the compared rates was 8 clicks per minute or less (half of these were four) and only 6 when it was greater (namely, 28, 48, 52, 55, 73 and 88 per minute); but there were only two doubtful judgments when the difference was 8 or less as against 16 when it was greater [namely, 16 (by four), 20 (by three), 28, 29, 32, 56, 60 (by two), 61, 64, 72, 77, 80, 81 (by two), 84, 96, 100, 108, 116 (by two), 120, 136 and 168 per minute]. One subject was responsible for nine of the doubtful judgments (Wa), two for six each (Co, E) and two for four each (Ko, Wy). This shows that only a few subjects felt uncertain about a preference, and reinforces the conclusion reached.

3. With objectively similar metronome clicks the preferred tempo for all subjects averaged .519 sec. (see Table I.). or approximately one half second (the median, .579 sec., is higher than the average).

There are, however, considerable individual differences in the preferences. The preferred rates ranged from 1.370 to .305 sec., which makes a difference of 1.065 sec. These are the extreme rates afforded by the instrument, so that it is possible that still slower or faster rates might have been selected. Neither of the extremes seems to possess any advantage. During the initial comparison between the extremes, 40 was preferred 11 times to 208 nine times. But

it is only rarely that either is finally selected; it happened only with 15 per cent. of these subjects. There were only two rates that were preferred twice each, namely .630 sec. (by E and McD) and .305 sec. (by Mo and T).

It is, therefore, difficult to trace any definite *central* tendency in respect to tempo preferences of very wide applicability—because of the varying factors that determine the choice, as we shall see—but we can recognize *types* of subjects. The results can be arranged readily into four groups, according as the preferred tempos exceed 1 sec., fall between 1/2 and 3/4 sec., and fall somewhat short of 1/2 sec. and of 1/3 sec. These four groups may be referred to as *slow*, *medium*, *fast* and *rapid*. The average speed for the slow is 1.169 sec; for the medium, .618 sec.; for the fast, .435 sec.; and for the rapid, .319 sec. Of these values, the medium interval length, which is approximately .60 sec., is evidently the most representative. It is obvious that the faster rates are preferred to the slower, since all but four selected a faster rate than .70 sec., and all except eight a faster rate than .61 sec. To what extent the arrangement of the steps on the metronome pendulum is responsible for these preferences was not determined. The metronome gave 24 speeds faster than .70 sec. (88 per minute) and only 6 slower. But when the records are analyzed it is found that the slower of the pairs of rates was selected in the aggregate by the twenty subjects 351 times, while the faster were selected only 280 times. Thus the 'weighting' of the fast end of the pendulum was overcome, very largely if not entirely, by the more abundant selections of the slower tempos.

If we accept a rate of from .50 to .60 sec. as the most probable value, or the value which will most frequently be obtained, for the preferred tempo, our result will harmonize with a number of earlier findings. Vierordt found a 'neutral' or 'adequate' interval which averaged .62 sec. Wundt's 'indifference' interval averaged .60 sec.; Stevens' interval which could be reproduced the most accurately ranged from .53 sec. to .87 sec. (Johnson, however, experimenting with practice and fatigue, found no indifference point from which

variations did not occur.) In speech it has been found that the average length of the 'complex centroid intervals'[1] is .58 sec.; and that speech series which consist of 'three-syllable sound centroid intervals,'[2] and which average .65 sec., are the most regular or periodic. Speech intervals, because speech is one of our constant psychomotor activities, ought to exercise an important influence upon the selection of the preferred rate. Reference may also be made to measurements of the most favorable period for subjective rhythmization which, according to Martius' result, is 0.50 sec. This coincides with our general average, while Meumann's favorable period for subjective rhythm is somewhat less, .40 sec., and Bolton's considerably more, 1 sec. This wide difference in the findings is probably dependent upon the varying nature of the factors which influence subjective rhythm (see points 4 and 5). Bolton found that subjective rhythmization did not occur when the interval was longer than 1.59 sec. If we assume that an interval will probably not be preferred if it is longer than the limit of subjective rhythmization, it will be noticed that the extreme rate employed in our metronome experiment falls about one fifth second short of the 1.59 sec. limit.

4. The factors which influence the preference for a given rate are numerous and vary more or less with the individual and with the circumstances of the occasion, which accounts for the wide difference found among various individuals. The introspections indicated that one or more of the following factors were operative with one or another observer: physiological irritation or pain, mental disquietude, annoyance, irritation, impatience, or repose; the strain of suspense or expectation, the effort of attention to follow, stimulation to movement, breathing, rhythmical tendency, harmoniousness or discord (tendency to harmonize with ideal beats, tunes, keeping time, preconceived rhythms, suggestions), temperament and associations and suggestions.

[1] The interval from one accented syllable to another, including pauses: cf. Wallin, 'Researches on the Rhythm of Speech,' Studies from the Yale Psychological Laboratory, 1902, IX., 118.

[2] Wallin, as cited, 104 and 110.

The most frequent objection against the slow, or very slow, rates were that they dragged and thus produced a feeling of suspense and a tendency to hurry (B, Mo, C, L, H, W, Wy and I). Three found that they irritated them or made them feel uneasy (K, Mo and Ea), one that they caused a pain in the ear (Mo), and one that they interfered with the breathing (Wa). It is noticeable that only one of these selected a rate faster than .705 sec. While three reported that the slow beats were smoother or firmer (M, J, Ko), and three that they gave a better sense of rhythm (M, Mo, Co), none of these selected a rate slower than .673 sec., while three selected 'fast' and 'rapid' rates.

The most frequent objections to the rapid, or very rapid, rates were that they were irritating or annoying (B, I, M, W), that they were too hurried (J, K, Wy), and that they required too much effort to follow (B, H, L). One found that they produced a headache (W), one that they 'hurt the head' (I), and one that they made the ear throb (Z). Two found them unrhythmical (J, C). They were jerky (Ko), discordant (C), or aroused unpleasant associations (Z, who says he is temperamentally slow), or it was difficult to carry a tune with them (Co). It is noticeable that three of these subjects, notwithstanding the objections, selected relatively fast rates. One found them livelier and more harmonious, and, consistently with these judgments, selected the most rapid tempo (Mo).

The subjects were asked whether they noticed any difference between the faster and the slower series. Three said that the faster seemed louder (K, McD, B), four the opposite (M, Mo, T, J), two that the faster were more distinct (B, Z), three that they were higher in pitch (T, E, Z), one that they made a different noise (I), and three that there was no difference (L, Wy, Ea). None of these factors seems to have exercised any influence upon the selection of the preferred interval.

Aside from the above factors which militated against the selection of rapid or slow rates, the following were the reasons given for selecting the particular rate that was chosen: it

caused the least disturbance or irritation (Wy, Ho), it was the easiest to follow (B, L), it seemed like the average time in music (McD), it gave the most pleasing rhythmical impression (L, who sought a rate that would 'fall into a chord'), it enabled the subject to carry two or three tunes (Co), it was more natural and enabled the subject to keep time to it better (I), it coincided with an 'ideal beat in the head' (Ho), and it fell in with the subject's temperamental disposition (Z).

Listening to the clicks frequently aroused suggestions, associations and images of various kinds which, no doubt, subtly influenced the preference. The faster rates suggested annoying machinery, pile drivers and steam engines, to Z and the slower a swinging pendulum, which was probably more pleasing, as he selected a slow rate. K also imaged a swinging pendulum, but he felt that it was too slow and selected a faster rate. Most frequently imaged was a clock, or the tick-tack of a clock. Of the five who did so (Wa did so only 'indirectly'), all chose slow speeds except one (Z, Ea, Wy, B, K). One imaged telegraph clicks and machinery, (Mo), and another the tick of a clock, which appealed to him, but also the clatter of horses in a race, which appealed more to him (Ko), and both chose rapid rates. One imaged through the test the stone cutters' hammers, chisels and clicks which he had observed before he had entered the laboratory; but for this he felt that he would have chosen a slower rate. He came into the laboratory with a certain set or attitude induced by the experience of the moment, and this determined his choice. One associated his preference with keeping time, singing, the movements of the hands of cheer-leaders and the heart beat, and accordingly chose a fast rate (I). One imaged a man pounding an anvil (Ea), one the ringing of a bell (Co) and one a machine (McD); what influence these suggestions had cannot be said, as the rates of operation for these objects may differ considerably. Sometimes the imagery will change. Thus E first visualized a figure eight motion for the slow ones; then (usually) the motions of a hammer, which sometimes struck higher, and once toward the close a curved hill, with the hammer moving from one part of the

hill to another. W once had an image of a man hammering on an anvil. When the beats were too slow or too fast they were not in unison. He also imaged a hammock; at the beginning he preferred long and slow swings (the records show that he selected the 40 rate three times) and a soothing rate; at the end the hammock was tossed so that it struck at both top and bottom, which made him feel seasick. It was then evidently moving faster; the rate which he selected was .687 sec. These instances indicate that form of imagery and the associations of the moment sometimes exercise a dominant influence upon the preference of tempos. Since the preferred rate depends upon so many and varied factors it seems doubtful whether we shall be able to lay down any *norm* for series of objectively similar sounds, except within wide limits.

5. Apparently there is no definable relation between any particular tempo preference and *musical capacity*, or the ability or inability to sing or play; but the preference seems to depend on the relative predominance of the melodic or harmonic elements or the rhythmic element in one's musical appreciation. Of the subjects who were interrogated, five said that they did not sing, and of these, three are in the medium group (W, E, McD), one in the fast (I), and one in the rapid (Mo). Five sang, two of these being in the medium group (J, M), one in the fast (Ko), and one in the rapid (T). Five sang a little, two being in the slow group (Ea, Wy), two in the medium (E, McD), and one in the fast (Mo). Of those asked, nine said they could carry a tune, three being in the slow group (Z could whistle a tune), two in the medium (J, E) and four in the fast (I, if a crowd was singing). One could not carry a tune (Mo, rapid tempo). Six played an instrument (usually the piano), two being in the slow group (Q, Wy), one in the medium (W), two in the fast (K; C, violin), and one in the rapid (Ho). Five played a little, three being in the medium group (B, Wa, L), one in the fast (Co), and one in the rapid (T). Of the seven who did not play, one was in the slow group (Ea), four in the medium (E, McD, M, K), one in the fast (I), and one in the rapid (Mo). There does not exist, therefore, any clear evidence of correlation between

the favored rate and the ability or inability to carry a tune, to sing, or to play.

A few of the subjects were asked to state which element they enjoyed most in music, the melody, harmony or rhythm. Three preferred the air or melody (Z in piano music), two of these selecting slow rates (Z, Ea), and one a medium rate (J). Two preferred the harmony (Z in orchestra music), both being in the slow group (Z, Wy). One, in the medium group, preferred these two elements to the rhythm (Wa). Three preferred the rhythm to either of the other two elements, and all of these selected fast rates (Co, I, Ko). We may therefore conclude that those persons who chiefly enjoy the melody and harmony in music will favor medium or slow tempos, and those who prefer the rhythm will select fast tempos.

### B. The Preferred Tempo as Determined by a Method of Expression (Timing the Spontaneous Responses to Music in Theaters)

In order to study the tempo preferences under the best conditions, we must have recourse to musical rhythms. Certain musical rhythms spontaneously evoke motor tendencies, such as swaying of the body, moving the hands or fingers, chewing (gum), and especially stamping the feet in time with the music. What rates of rhythm or musical tempos tend to produce the most hearty or vigorous motor expressions, the loudest or most diffuse stamping in an audience of unsophisticated people? It may be assumed that the more vigorous the motor responses for a given rhythmical rate, the stronger is the preference for that particular rate.

In order to study the problem by this method to the best advantage, frequent visits were made to the galleries of theaters. The galleries are usually peopled by men and boys who, unaffected by the conventional restraints of the cultured theater patron, allow their feelings to find vent in spontaneous expressions. Accordingly the responses (stamps of the foot) made to different vocal and instrumental selections and stage dances were timed by means of a stop watch, or, in some cases,

an ordinary watch. The timing continued as long as the responses continued at the same intensity for a given selection. The total number of seconds elapsed was divided by the total number of stamps counted, in order to obtain the average interval length between the stamps, *i. e.*, the average tempo. It is possible that in some cases there was more than one response to a measure, so that the rate of the stamps may not always coincide with the duration of the measure, but the records were not made carefully enough to make it possible to work out the distinction with accuracy. The writer also regrets that, having only a slight expert knowledge of musical technique, he is unable to detail the results for the different kinds of musical measures or the different varieties of movements. Nothing has been attempted in this direction beyond grouping separately some of the 2/4 and 3/4 times (Table IV.). It is possible that there may be some inaccuracies in this grouping, as the writer was not always able to recognize the patterns with complete certainty. The problem invites further attention from a psychologist with a technical musical training. There remains to consider the relation of different tempo preferences to different kinds of vocal and instrumental music, to different kinds of dances, to different kinds of measures, and to different kinds of movements of the same measures. In this study the responses were timed during vaudeville, burlesque, minstrel, light opera and glee club performances.

*Results.*—1. It was possible to grade the responses into four classes, according to the loudness of the stamps or the pervasiveness of the tendency (*i. e.*, the number of persons who fell to beating time). Sometimes the tendency was pervasive although the stamps were not especially loud. These grades are referred to in Table III., as very good (I., a vigorous or pervasive tendency to stamp), good (II.), fair (III). and poor (IV., a feeble tendency to beat time). The average speeds of the responses for these grades are, respectively, .51, .56, .59 and .66 sec., and the general average .58 sec. This gives a difference between the successive groups of .05, .03 and .05 sec., or a difference of .15 sec.

between the best and poorest responses. The differences, in all cases small, are approximately constant from group to group.

2. It is noteworthy that the average for the most vigorous responses evoked by music corresponds precisely with the average for the preferred tempo with metronome clicks (.51 sec., cf. p. 210). It also appears that the averages for the four grades of response to the musical rhythms (from .51 to .66 sec.) correspond quite closely with the most probable value found above for the preferred tempos with metronome clicks, viz., from .50 to .60 sec. (p. 211).

3. Owing to the fact that the large and the small counts may not precisely neutralize one another in the general average, the relative distribution of the averages in ten seconds groups may furnish better indices of the preferred rates than general averages. A comparison of columns (1) to (10) in Table III. shows that the highest number of averages in any of the ten seconds groups is forty; viz., for the rates lying within .41 and .50 sec. This is followed by 32, for the .51 to .60 sec. group, and 26, for the .71 to .80 sec. group. Only ten averages come between .30 and .40 sec.; only twelve between .81 and .90 sec.; and only four between .91 and 1.00 sec. More averages (viz., 50) lie below .50 sec. than above .70 sec. (viz., 42), while the largest number comes between .51 and .70 sec. (viz., 55). It will be noticed that, although there is a large number of averages coming within our most probable value for metronome clicks (.51 to .60 sec.), the greatest number comes within the limits of .41 and .50 sec. This applies particularly to the best grade of responses, grade I., where fifteen averages come within these limits, as against nine between .51 and .60 sec., and none whatever above .80 sec., and where the majority of the averages lie between .41 and .60 sec. (viz., 24, as against a total of 18 for all the other groups). For both the second and third grade of responses, the majority of the averages are contained in the three groups extending from .41 to .70 sec. For grade II. 24 averages come within these limits, as against 18 elsewhere; and for grade III. the proportion is 22 to 8.

These facts, therefore, would seem to warrant the conclusion that the most probable limits within which the preferred tempo will come are .41 to .70 sec. This is larger by about ten seconds on either side than the most probable limits found for the metronome clicks (p. 211). All the above facts, when considered in the light of the additional experimental data cited on page 211, indicate that for most persons under oridnary conditions *auditory rates or musical tempos varying in speed from .40 to .70 sec. will be preferred*, with a pronounced tendency to choose rates toward the faster end of these limits. That the faster rates call out the strongest responses, or preferences, is indicated particularly by the distribution of the averages in the best grade of responses, grade I. We have already pointed out that the tendency with metronome beats is likewise to prefer the faster rates (p. 211).

4. As with metronome clicks, there is a considerable range between the extreme rates which evoked responses. The fastest responses measured averaged .27 sec. (grade III.), and the slowest 1.00 (grades II. and III.). Here the range, however, is smaller than the range for the clicks (from .305 to 1.37 sec.). The fact that fast and slow rates are found in all grades indicates that the preference, or the vigor of the response, is not dependent entirely upon the tempo. The catchiness and familiarity of the music, the distinctness or incisiveness of the accent and the character of the measure, are also important factors. Musical airs which are not catchy will incite to less response than catchy selections, although the tempo is the same. Tempos with barely perceptible accentuation will produce feebler motor reactions than rhythms with a decided accent. Likewise the swing required in waltz music demands a tempo which would be forbidding with other musical patterns. The responses for 3/4 time are uniformly slower than for 2/4 time (Table IV.), the differences for the corresponding grades amounting to .14, .11 and .14 sec. The slowest recorded average for the 2/4 time, .53 sec. for grade III., is faster than the fastest average for the 3/4 time, .62 sec. for grade I. The best rate for 3/4 time, according to these results, is .62 sec.; and for

the 2/4 time, .48 sec. Most of the former patterns were probably waltz times, and most of the latter two-steps.

There are also other factors which account for the distribution of the same tempos in the different grades of response. Sometimes the measurements were made under unfavorable conditions; rainy nights at times made the audience small and unresponsive; and sometimes there were few children present. Tempos which were rated as low on these nights would probably have received a higher rating if the conditions had been propitious.

## Table I

Preferred Tempos with Metronome Rates (Final Selection)

| Subject | Beats per Minute | Corrected Length of Intervals in Seconds |
|---|---|---|
| Ho | 179 | .348 |
| K | 110 | .566 |
| M | 108 | .577 |
| McD | 98 | .630 |
| C | 156 | .395 |
| Mo | 208 | .305 |
| B | 88 | .705 |
| T | 208 | .305 |
| L | 116 | .536 |
| W | 90 | .687 |
| H | 107 | .582 |
| Wa | 104 | .60 |
| J | 92 | .673 |
| Co | 128 | .484 |
| E | 98 | .630 |
| Ea | 48 | 1.18 |
| Z | 40 | 1.37 |
| Ko | 144 | .428 |
| Wy | 63 | .958 |
| I | 142 | .434 |
| Ave. | | .519 |

## Table II

Preferred Tempos Arranged into Four Groups

I. *Slow*

| Subject | Corrected Interval Length Seconds |
|---|---|
| Z | 1.370 |
| Ea | 1.18 |
| Wy | .958 |
| Ave. | 1.169 |

II. *Medium*

| | |
|---|---|
| B | .705 |
| W | .687 |
| J | .673 |
| E | .630 |
| McD | .630 |
| Wa | .600 |
| H | .582 |
| M | .577 |
| K | .566 |
| L | .536 |
| Ave. | .618 |

III. *Fast*

| | |
|---|---|
| Co | .484 |
| I | .434 |
| K | .428 |
| C | .395 |
| Ave. | .435 |

IV. *Rapid*

| | |
|---|---|
| H | .348 |
| Mo | .305 |
| T | .305 |
| Ave. | .319 |

## TABLE III

RHYTHMICAL RESPONSES (STAMPS) TO MUSIC IN THEATERS

| Grade. | No. | Ave. Sec. | S | L | R | Distribution of Averages. | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | | | | | | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
| I., V.G. | 42 | .51 | .34 | .76 | .42 | 7 | 15 | 9 | 5 | 6 | 0 | 0 | 22 | 6 | 14 |
| II., G. | 49 | .56 | .40 | 1.00 | .60 | 1 | 12 | 11 | 10 | 7 | 5 | 2 | 13 | 14 | 21 |
| III., F. | 32 | .59 | .27 | 1.00 | .73 | 1 | 7 | 9 | 6 | 4 | 1 | 2 | 8 | 7 | 15 |
| IV., P. | 27 | .66 | .40 | .90 | .50 | 1 | 6 | 3 | 2 | 9 | 6 | 0 | 7 | 15 | 5 |
| Ave. | | .58 | | | | | | | | | | | | | |
| Total | | | | | | 10 | 40 | 32 | 23 | 26 | 12 | 4 | 50 | 42 | 55 |

Grade, grade of response: I., very good (loud stamps); II., good; III., fair; IV., poor (weak stamps). No., number of series measured. The shortest series contained seven stamps; the longest 108; the majority, from 30 to 50 stamps. Ave., average time of interval between the responses, in seconds.

S, shortest average for any series. L, longest average. R, range between the longest and shortest averages.

(1) Number of averages falling between .30 and .40 sec. (2) Ditto, between .41 and .50 sec. (3) Do., between .51 and .60 sec. (4) Do., between .61 and .70 sec. (5) Do., between .71 and .80 sec. (6) Do., between .81 and .90 sec. (7) Do., between .91 and 1.00 sec. (8) Do., below .50 sec. (9) Do., above .70 sec. (10) Do., between .51 and .70 sec.

## TABLE IV

| Grade. | Time of Responses in ¾ Time. | | Time of Responses in ¾ Time. | | |
|---|---|---|---|---|---|
| | No. | Ave. Sec. | No. | Ave. Sec. | D |
| I., V.G..... | 13 | .62 | 15 | .48 | .14 |
| II., G. ..... | 20 | .64 | 9 | .53 | .11 |
| III., F. ..... | 12 | .67 | 4 | .53 | .14 |
| IV., P. ..... | 13 | .73 | 0 | | |

Explanation of signs: see Table III. D, difference (amount by which ¾ is shorter than ¾ in the corresponding grade).

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